LETTERS TO THE EDITOR.

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An Electromagnetic Problem.

IN the application of general principles to special cases it is sometimes found that the result is a seeming paradox, which is not always easy to remove. Such problems, although involving no new principle, are nevertheless of considerable interest, and after attaining their satisfactory solution we often realise that we did not before appreciate the full import of the general law.

The following question has been discussed with considerable interest among some of the writer's friends, and therefore it seemed not improbable that other physicists might be interested.

If two spheres of positive electricity are near together and are suddenly released, it is clear that their potential energy decreases as they separate and goes over into kinetic energy of motion. This kinetic energy is, of course, the energy of the magnetic field which results from the motion of the charges.

It seems possible, however, to arrange a system so that this magnetic field shall vanish because of symmetry, and the question then presents itself, Where is the energy? Suppose we have a sphere of positive electrification placed as the water is in a soap bubble, and capable of expanding under the mutual repulsion of its parts. The potential energy of the electricity certainly decreases as the sphere expands, and *if the electricity be considered continuous* there is certainly no chance for a magnetic field, as is easily seen from consideration of symmetry. If the sphere be allowed to expand, where does the energy go? The obvious answer is that the electricity is not continuous, but exists as discrete particles, *i.e.* as electrons; but if we try to escape the difficulty in this way, it is equivalent to admitting that the electrical laws, together with the conservation of energy, *require in themselves* the discrete structure of electricity. If, on the other hand, we say that the electricity is associated with matter, *i.e.* with ponderable mass, and the energy appears as ordinary mechanical energy of motion, then we are admitting that the electrical and energy laws *require the association of electricity with matter.*

There seem to be no other solutions to the problem than those above given, and if we admit either of them we reach a conclusion which certainly is striking when we consider that we have only used the general laws of electricity and energy.

The writer does not state the above as a fundamental paradox, but only as an interesting problem.

D. F. COMSTOCK.

Institute of Technology, Boston, Mass., November 3.

The Progress of Aviation.

I HAVE read with great interest the article on the above subject by Prof. Bryan in NATURE of October 29.

May I be permitted to direct especial attention to the necessity for finding the displacement of the centre of pressure on all kinds of surfaces and at all angles therein referred to? The paper by Prof. Bryan and Mr. Williams on the subject of longitudinal stability, and Captain Ferber's article in the *Revue d'Artillerie* (November, 1905), both assume the truth of Joëssel's law. There is, however, every reason to suppose that there is a certain critical angle below which Joëssel's law ceases to be true, the displacement decreasing with the angle instead of increasing.[1]

Consequently, the numerical conclusions arrived at from the stability formulæ of Captain Ferber and Prof. Bryan may be very wide of the mark.

I hope to experiment in this direction myself, but my time is very limited. There can be no doubt whatever that a thorough investigation as to the centre of pressure would be of the greatest practical use.

HERBERT CHATLEY.

32 Britannia Road, Southsea, October 31.

I AGREE strongly with all that Mr. Chatley has said. It cannot be too emphatically pointed out that the object of our stability investigations was to show that the subject is capable of being treated mathematically, and that, *given the requisite experimental data,* the conditions of stability of any system of planes or surfaces can be calculated out in the form of numerical results. The cases in which this was done were intended merely as *examples* illustrative of the general method, and for this purpose Joëssel's law furnished the simplest assumption available at the time. It will be noticed, too, that arbitrary values were assumed for the moments of inertia of the systems. To draw inferences from the results of examples worked out with this object would be an unfortunate mistake.

It is to be regretted that want of time has prevented my attempting to work out any examples based on the Turnbull results, though the idea suggested itself when I saw the paper in the *Physical Review*. The theory of stability has thus been somewhat at a standstill. Those who, like Mr. Chatley and myself, would like to see that theory advanced are prevented from doing this by pressure of other duties, while those who have the necessary time and money have been mainly occupied of late in breaking records. Mr. Lanchester's theory of stability starts from so different a standpoint that it must be discussed at a future time.

G. H. BRYAN.

Potato Black Scab.

THE discovery this autumn of black scab in the potato crop in two localities in co. Down was the means, through the Irish Department of Agriculture, of supplying me with excellent material of diseased tubers for examination. I have kept the resting "spores" of the chytridian fungus *Chrysophlyctis endobiotica*, Schilb., causing the disease, under varied conditions of temperature, nourishment, moisture, and light, and have succeeded in causing the "spores" to germinate, especially by cultivation in potato juice. Each "spore" proves to be a zoosporangium, full of zoospores or zoogonidia, seen in active swarming motion before rupture of the sporangium. The zoospores, 1·5–2 μ in diameter, escape through a slit-like opening in the wall of the sporangium 30–60 μ in diameter, and have the usual characters of a chytridian zoospore.

Since the publication of Schilbersky's short preliminary account in 1896 in the *Berichte der deütscher botanischen Gesellschaft*, and Potter's account of his discovery of the pest in Cheshire in 1902, we have learnt nothing of the life-history of this injurious fungus.

T. JOHNSON.

Royal College of Science, Dublin, November 17.

The Nature of γ Rays.

EXPERIMENTS by Prof. Bragg and myself upon the secondary kathode radiation which proceeds from matter through which γ rays are allowed to pass, taken in conjunction with the similar result announced by Mr. Cooksey in NATURE of April 2 (vol. lxxvii., p. 509) for X-rays, support the theory of the material nature of X and of γ rays originally advanced by Prof. Bragg.

The modification of the ether-pulse theory recently advanced by Prof. Thomson may possibly furnish a partial explanation of these effects, but in the light of some experiments which I have lately carried out upon the secondary γ rays, even this modification seems quite insufficient. A brief summary of these results is appended.

(1) The γ rays of Ra, and probably of Th, appear to consist of two distinct homogeneous bundles, the value of λ/Δ (where λ is the absorption coefficient and Δ the

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[1] Spratt-Moedebeck's "Pocket-book of Aëronautics" (1901); Wilbur Wright, Smithsonian Report, 1902, pp. 133–148 (*Journal of Western Society of Engineers*, December, 1901); Turnbull, *Physical Review*, vol. xxiv., No. 3, 1907.

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density of the material) for the soft set being approximately four times that for the hard.

(2) For each set of rays the value of λ/Δ is constant, and practically independent of the nature of the absorbing material with which λ is measured, provided that in the case of the soft rays secondary effects be excluded.

(3) Secondary γ radiation appears on both sides of a plate which is penetrated by a stream of γ rays. There exists a marked lack of symmetry between the amount of secondary radiation which proceeds from the two sides.

(4) A lack of symmetry exists in the case of some substances between the quality of the radiation on the two sides.

(5) The last results seem very difficult to reconcile with a pulse theory. On the "material" theory propounded by Prof. Bragg no such difficulty arises.

(6) The secondary γ radiation appears to be derived from the primary by a process of scattering, this process generally involving a reduction in the subsequent penetrating power of the ray affected.

(7) There appears to be reason to believe that the distribution of the scattered radiation depends to some extent upon the hardness of the radiation which is scattered, also upon the nature of the material in which the scattering is produced. The softer radiation appears to be turned back to a somewhat greater extent than the hard. Materials of high atomic weight seem to be able to produce more complete scattering than those of lower atomic weight.

(8) The absorption of γ radiation which has already passed through a thickness of one substance by screens of a different substance may not in all cases give a true measure of the absorption of the original radiation which has been effected by the first screens.

J. P. V. MADSEN.

University of Adelaide, October 1.

[As there are few opportunities in Australia for an investigator to place his views quickly before a scientific public, we print the above letter, but with it the correspondence must cease. The subject is more suitable for discussion in special journals devoted to physics than in our columns.—ED. NATURE.]

The Origin of Spectra.

THE very interesting observation of the anomalous dispersion of luminous hydrogen in the neighbourhood of the Hα line recorded by Messrs. R. Ladenburg and Stanislaw Loria in NATURE of November 5 (p. 7), and the known absence of the phenomena in ordinary hydrogen, show conclusively that *the spectrum lines of a substance are not free periods of the atoms in their normal state, but only of those systems produced somehow by the agency which gives rise to the spectra.*

The figure 1/50,000 as the number of electrons per atom of course means that in the gas under experiment only one atom in 50,000 was emitting the Hα line at any one time. The very important remark is made that the anomalous dispersion in the neighbourhood of the other lines of the hydrogen series "is expected to be much smaller than that at the Hα line." If this be so, it will show that at any given time different numbers of atoms are producing the different lines, that is to say, that the spectrum is not produced *in toto* by each atom. Each atom (or rather the system emitting the lines) may, for instance, only be emitting one line at a time. These results are the same as those I have deduced from Prof. R. W. Wood's work on the anomalous dispersion of sodium vapour. Sodium vapour shows anomalous dispersion in the neighbourhood of all the lines of the principal series, which "is very strong at D, feeble at the first pair of ultra-violet lines λ 3303, and almost imperceptible at λ 2852." It is also, Wood states, stronger at D_2 than at D_1. This shows that the number of atoms emitting D_2 at any time is greater than the number emitting D_1, and both these are much greater than the numbers emitting the higher members of the series. We note that there is no anomalous dispersion in the neighbourhood of the lines of the subordinate series of the sodium spectrum showing that heat alone does not produce those systems which vibrate with the periods of the subordinate series, which agrees with the facts that these series do not appear in the absorption spectrum of sodium vapour or in the Bunsen flame spectrum of sodium.

It thus seems probable that different series of lines in a spectrum are produced by entirely different vibrating systems, while any system possibly only emits one line at a time of its own particular series, depending upon the manner in which it has been struck. It is evident that the different vibrating systems obtained, and their relative proportions, may be expected to vary with the nature of the electrical discharge producing the spectra, and hence the variation of the spectra under different conditions. This may, perhaps, on the modern views, be regarded as the same idea put forth many years ago by Sir Norman Lockyer in his dissociation hypothesis.

I make these observations in order that those working on the subject from the theoretical side may the better see the phenomena to be explained, which are quite different from ordinary dynamical vibrating systems.

In conclusion, I should like to direct attention to the importance of extending Messrs. Ladenburg and Loria's work. By examining every line in the spectrum of an element we could, for instance, say whether a line was faint because very few systems were emitting it, or whether its faintness must be attributed to the fact that the vibrations producing this line have only a very small amplitude.

ALBERT EAGLE.

Imperial College of Science and Technology,
London, November 9.

A Gall-producing Dragon-fly.

WHEN looking through Dr. C. Houard's new work on galls ("Les Zoocécidies des Plantes d'Europe et du Bassin de la Méditerranée," tome i.), I was surprised to find on p. 249 an entry :—"Minime borselette *Q. ped. Lestes viridis*, Van der Lind."

A gall-producing dragon-fly was quite new to me, but on looking up the subject I found a series of very important observations on the oviposition and larva of the species in question by the Abbé Pierre and M. de Roequigny-Adanson, in the *Revue scientifique du Bourbonnais et du Centre de la France*, xv. and xvi. (1902–3), and the *Annales et Bulletin de la Société entomologique de France* for 1904. As these seem to have been entirely overlooked in England, I think it may be useful to epitomise them as briefly as possible.

The eggs of *Lestes viridis* are laid on the branches of a great variety of deciduous trees and shrubs, but always close to, or overhanging, water, and therefore probably most often on alders or willows. These result in the production of small galls, which are sometimes extremely abundant, and which are thus described by Pierre :—

"Un bourrelet mesure de 1 mm. $\frac{1}{2}$ à 2 mm. de longueur, sur $\frac{1}{2}$ ou 1 mm. de largeur. Deux bourrelets sont associés en chevron et forment un angle d'à peu près 90°, ouvert vers le bas du rameau. Le sommet de l'angle présente une pellicule corticale plus ou moins arrondie, formant clapet au dessus de l'ouverture par laquelle de 1 à 4 œufs ont été insérés sur chaque bourrelet. Enfin les chevrons distants de 2 mm., sont associés en série longitudinale, de telle façon qu'une même génératrice du rameau soit sensiblement bissectrice de tous les angles."

The emergence from the eggs and the structure of the larva are equally curious. The new-born larva, or "pro-larve," as Pierre calls it (*Ann. Soc. Ent. de France*, 1904, pp. 477–84, pl. iv.), resembles a coleopterous pupa, being enclosed in an outer membrane which leaves it only the power of leaping. If these young larvæ do not fall into the water on emerging from the egg, they leap about, sometimes for several hours, until they succeed in reaching it. After reaching the water the pro-larva rests on its back for two hours, and then casts the skin, a process occupying from three to thirteen minutes. The larval development of *Lestes viridis* has been compared by M. Giard to that of the crickets. A similar structure of the newly emerged larva has also been noticed in *Epithera bimarulata*, another dragon-fly.

I may remark that *Lestes viridis*, though common on the Continent, is an insect of great rarity with us, and not firmly established in the list of British species.

W. F. KIRBY.

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